CONFERENCE ON GLACIOLOGICAL RESEARCH

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CONFERENCE ON GLACIOLOGICAL RESEARCH

New York City, January 18 and 19, 1949

SCHEDULE OF MEETINGS

Tuesday, January 18, 8:30 P. M. - Joint Meeting of the Arctic Institute of North America and The American Geographical Society in the auditorium of the Engineering Societies Building, 29 West 39th Street.

Introduction by Dr. Laurence M. Gould, President of Carleton College.

Address by Walter A. Wood: 'Project "Snow Cornice"'. Account of the Arctic Institute's expedition of 1948 to the Seward Ice Field in the St. Elias Mountains of Alaska and the Yukon to establish a high-level research station for glaciological and meteorological investigations and the working out of various techniques of operation and problems of logistics.

Wednesday, January 19

2:00 P. M. - Conference in the Arctic Institute Office in the building of The American Geographical Society, Broadway at 156th Street. (An outline of the discussion follows, beginning on page 2.)

8:00 P. M. - Meeting at the American Alpine Club, 113 East 90th Street.

Speakers:

Dr. Richard Foster Flint, Professor of Geology, Yale University; "The American Alpine Club as an Auxiliary in Glacial Research".

Dr. James L. Dyson, Professor of Geology, Lafayette College; "The Glaciers of Glacier National Park".

Maynard M. Miller, Research Associate in Glaciology, American Geographical Society; "The Juneau Ice Field Research Project, 1948".

Glaciological Conference, Wednesday, January 19, 1949, in the offices of the Arctic Institute of North America at the American Geographical Society, Broadway at 156th Street, New York 32, New York.

ATTENDANCE

(Listed alphabetically)

Dr. Henri Bader, Bureau of Mineral Research, Rutgers University; formerly at the Weissfluhjoch Snow and Avalanche Research Station, Davos, Switzerland.

P. D. Baird, Director Montreal Office, Arctic Institute of North America; Secretary of the International Commission of Snow and Ice of the International Association of Hydrology.

Dr. Walter H. Bucher, Professor of Geology, Columbia University; President, American Geophysical Union.

Milton Dobrin, Lecturer in Geology, Columbia University.

Dr. James L. Dyson, Professor of Geology, Lafayette College.

William O. Field, Jr., The American Geographical Society; Chairman, Committee on Glaciers, Section of Hydrology, American Geophysical Union.

Joel Ellis Fisher, The American Alpine Club.

Henry S. Hall, Jr., The American Alpine Club.

Dr. Serge A. Korff, Associate Professor of Physics, New York University.

Robert Lange, geology student at University of New Hampshire.

Dr. Richard U. Light, President, The American Geographical Society.

Nelson McClary, Ship's Mate, Ronne Antarctic Research Expedition.

Maynard M. Miller, Research Associate in Glaciology, The American Geographical Society.

ATTENDANCE (Cont'd.)

Dr. Robert L. Nichols, Professor of Geology, Tufts College.

Mrs. Philip Dana Orcutt, The American Alpine Club.

Frank Press, Lecturer in Geology, Columbia University.

Dr. Louis L. Ray, U. S. Geological Survey.

Dr. M. C. Shelesnyak, Office of Naval Research.

Dr. Arthur N. Strahler, Asst. Professor of Geomorphology, Columbia Univ.

Andrew Thompson, graduate student in geophysics at Columbia University; Geophysicist of Ronne Antarctic Research Expedition.

Dr. A. L. Washburn, Executive Director, Arctic Institute of North America.

Walter A. Wood, Director of New York Office, Arctic Institute of North America.

J. Lamar Worzel, Research Associate in Geophysics, Columbia University.

Dr. Richard U. Light opened the meeting and welcomed those assembled on behalf of The American Geographical Society. He then turned the meeting over to Mr. Field who acted as Chairman.

Mr. Field stated that, so far as known, this was the first such meeting to discuss glaciology ever to be held in the United States and expressed the hope that it would become an annual event. A number of messages were read expressing regret at not being able to attend and extending best wishes for the success of the conference. A telegram of greeting was received from Dr. R. F. Legget of the National Research Council of Canada and Dr. J. Tuzo Wilson, Professor of Geophysics, University of Toronto.

It was the sense of the meeting that words of greeting from those assembled should be sent to glaciologists on other continents, among whom were Dr. Hans W:son Ahlmann in Stockholm, Dr. J. A. Broggi in Lima, Peru, and various members of the British Glaciological Society.

The Chairman spoke of several projects now underway or in the planning stage which involve glaciological studies, and stated that he hoped the discussion at this meeting would be of help in suggesting new lines of investigation and methods to be applied to field techniques. In this connection, he recalled the very fruitful discussion of the Association for the Study of Snow and Ice in England in 1939 upon which Professor F. Alton Wade to a considerable extent based the glaciological program of the U. S. Antarctic Service Expedition of 1939-1941, and expressed the opinion that conferences such as these ensure more closely coordinated studies and an opportunity for different expeditions to benefit more directly from the experience of those preceding.

The first speaker was Mr. Baird who gave a brief report of the meeting of the International Association of Hydrology at Oslo in September, 1948, at which he had been elected Secretary of the International Commission of Snow and Ice. He described the organization of the meeting, the papers presented, and the more important resolutions which were adopted. One of the questions set for special study over the succeeding three-year period, to be reported on at the next meeting of the Commission in Brussels in 1951, was the flow mechanism and structure of glacier ice. Also of interest was a resolution that the present Committee of Glacier Measurements should continue the yearly measurements of the length of the greatest possible number of glaciers and also enlarge its scope to include yearly three-dimensional measurements on selected glaciers.

Dr. Bader was the second speaker and was followed by Dr. Bucher who spoke extemporaneously, using lantern slides and diagrams on a blackboard. Dr. Bader's statement and a written summary of Dr. Bucher's remarks follow.

Statement of Dr. Henri Bader

I have been asked to speak on present trends in European glaciology, but am yet unprepared to do so explicitly. However, the newer European views are in part reflected in what I shall say. Before I begin I would like to make it clear that I do not speak with any authority on matters glaciological. I am primarily a snow man and a newcomer in the study of the denser ice. Also I have not stood on a glacier for more than ten years. I venture to speak only because I believe that my attitude towards glaciology, as inspired by snow research, is partly shared by the new school emerging in Switzerland from the Federal Institute of Snow and Avalanche Research and in England from the Cavendish Laboratories.

Our knowledge of glaciology is very largely descriptive. We know quite well what glaciers do geologically. We know less well how they do it, and we know not at all why they do it, in terms of ice mechanics.

Thus it appears that if we are to make significant progress in glaciology beyond a largely morphological description of the world's present glaciers, we must study the behaviour of ice masses as such. This is particularly necessary if we wish to estimate the thickness of Pleistocene ice sheets with some degree of confidence.

We are here confronted with several distinct problems, some of which can be studied in the field, and others which can only be solved by laboratory work.

We have an incomplete, but satisfactory picture of the transformation of snow into firn, and of the manner in which firn becomes dense up to a certain point. But the transformation of firn into ice seems, at least to me, wholly mysterious. At a density of somewhere around 0.7 to 0.8 the pores in the firn close and the remaining air can no longer escape. Yet most of it apparently does escape,* for glacier ice has a higher density, and we do not observe any significant compression of the air bubbles in the upper parts of glaciers. It is unfortunate that none of the pits dug in the firn regions have reached real ice, and we do not seem to be able to obtain much information by way of crevasses, although here there are indications of a sudden change from firn to ice. This would support the view that ice is formed by infiltration of melt water down to already impermeable ice, and by subsequent freezing. But there are two difficulties; one is the disposal of the heat of congelation and the other springs from the Antarctic and high Arctic, where the liquid phase cannot be of any importance near the surface. The transition of firn to ice is typically a field problem. We can do very little in the laboratory until we have more field data for guidance.

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* As a consequence of field observations (summer 1949) and new theoretical considerations, I now (winter 1949/50) believe that enclosed air cannot escape from glacier ice.

A great deal of work has been done on the mechanical properties of ice, but I venture the opinion that much of it is of little value because it is usually not made sufficiently clear what kind of ice has been used in the determination of numerical values of some property. To state that artificial ice, or lake ice, or glacier ice was used, is not sufficiently descriptive of the material under consideration.

A special problem is presented by the glacier ice itself. It is hardly correct to speak of glacier ice without further qualification. There are different types, such as a dense black ice, gray ice, white ice, ice loaded with debris, blue bands, etc., and it is not unreasonable to suspect that their mechanical properties may differ considerably. They may react to stress with different modes of deformation. The problem of the formation of the different types of ice is one for field study alone at the present time.

We now come to the main problem confronting the glacial physicist, if I may use the term among geologists, and that is the motion of the glacier. Here we have a very large accumulation of observations, direct observations of surficial motion and inferences on basal motion. I have recently read a statement that there are some 80 different hypotheses of glacier motion. Now, while at least nine-tenths of these have been discredited, they do reveal a state of very large discrepancy between empiricism and theory.

Inadequate theoretical knowledge for any length of time is nearly always indicative of inadequate observation and experimentation, and in our case the inadequacy is not quantitative but qualitative. We do not have the right kind of observation, nor the right kind of experimental procedure. So one of the first tasks is to discuss and formulate the type of observations that should be made in the field, and then to survey the limitations imposed by technique and finance. It seems clear that we must determine velocities and accelerations inside the glacier; we must observe what kinds of ice are moving in different parts of the glacier and how they are moving. This is quite a formidable task, and there is little doubt that the deeper parts of large glaciers will be permanently out of our direct reach due to the presumably high plasticity of the ice under great hydrostatic pressure.

All this, however, represents only the beginning of systematic research, because the analysis of stress is probably impossible on the basis of knowledge of the main deformational processes in a glacier alone. We must also study the pertinent mechanical properties of the different types of ice as determined under controlled conditions. This makes laboratory work unavoidable.

Now here we will have to proceed very carefully. In some cases we will have to speed up deformational processes in order to obtain the necessary information within a reasonable time, and in other cases we will have to slow them down because we cannot exert the necessary large

stress or work on sufficiently large samples. A conceivably very important complication may result from preferred orientation of the crystals in an ice aggregate. I suspect that we will find for a given grain size, grain size and shape distribution, pattern of grain orientation, hydrostatic pressure, temperature, and whatnot, that there will be a maximum of what may be called "readiness for yielding to stress" only when the stress is in the right direction and has the right magnitude. We would then find a minimum viscosity. Any other stress, applied in any other direction would result in a lesser deformation and fool us into accepting a higher value for viscosity.

Now the structure and orientation pattern of the ice from any portion of the glacier is the result of the physical and particularly the mechanical history of the specimen. In the upper reaches of the glacier, where motion is slow and stress changes gradually, the ice may have time to change its structure continuously and always respond with maximum deformation to the prevailing stress. In the lower reaches, velocities are higher and stresses change suddenly. The structure cannot change fast enough, deformational behaviour becomes erratic and anything can happen: such as, for instance, stiffening and upsurge. Possibly I am talking nonsense, but it is a fact that the results of laboratory work on ice by different men show a very abnormally wide range for the value of viscosity. We should be very much disturbed by the reported values for viscosity of glacier ice at the melting point of between 10^{12} to 10^{14} poises. Further work should not be done without careful microscopical determination of the structure of the ice specimens to be tested. There is no doubt that ice mechanics requires a highly specialized laboratory technique, some of which is being developed at present at Davos, Switzerland, and Cambridge, England.

To anyone intending to set up a glacier ice mechanics laboratory I humbly offer the following advice: induce a young physicist to absorb the scattered literature on the thermal regime, dynamics, and thermodynamics of firn and ice masses, and to report on his conclusions, which can be significant because the heat developed by moving ice is easily computed and because the coeficients of thermal conductivity are not greatly variable and are perhaps sufficiently well known. Such a study would be an extremely helpful guide in determining the kind of ice mechanics which should be done in the laboratory. Furthermore, it cannot be over-emphasized that the laboratory must be located as near as possible to the source of the material to be examined. The structure of stored ice specimens can change very rapidly by recrystallization (?).

I am at present gathering information to be able to report on new trends in European glaciological research.* The information is coming through distressingly slowly. European glaciologists appear to be too

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* This report was subsequently published in Bull. Geol. Soc. of America, vol. 60, No. 9, September 1949, pp. 1309-1313.

busy searching their glaciological souls to answer letters. This soul-searching seems to be resulting in a departure from what may be called the geological tradition in glaciology. Take almost any glacier and what have we got? A more or less lengthy description, a few photographs, and some hasty measurements made before the end of the season makes us fold our tents. Next year we go to another glacier. There are of course some very notable exceptions which have given us an inkling of the complexity of the mechanical and regime problems.

Roughly stated, the new idea that is taking shape, is for a research group to adopt a single appropriate glacier and then settle down and live with it, season after season, or if possible all the year round. The members of the group may change, but the group remains faithful to the same glacier. Thus, in a decade or two, we may hope to understand things which today are quite beyond our grasp.

The mandate is quite clear: Finance and organize long-term coordinated field, laboratory, and theoretical studies of one or several ice masses.

Arctic and Antarctic expeditions, especially when of a military nature, offer the worst possible conditions for research. In describing last summer's work in Alaska, Professor Sharp says that two-thirds of the time was spent keeping alive and moving personnel and supplies. I also gather that much of the remaining time was spent learning the hard way that glaciological field techniques are pitifully inadequate. Why not develop and test new methods on more easily accessible glaciers?

We are manifestly methodically unprepared for Arctic ice work and will remain so for a long time unless we change our approach. Arctic exploration and reconnaissance serves a definite glaciological purpose. It enables us to formulate the problems but furnishes little towards their solution. Systematic research must be organized on a different basis. The establishment of an ice mechanics laboratory near glaciers is mandatory.

Summary of Dr. Walter H. Bucher's Remarks

Where circumstances are favorable, much of value can be learned about the transformation of the névé of a snow field into the ice of a glacier through careful mapping and study of the structural features that lie exposed on the surface of the glacier, with the methods used in modern structural geology. The processes by which the broadly lens-shaped body of the névé of a catchment basin is transformed into the narrow semi-cylindrical shape of a valley glacier are not only analogous, but on the whole, dynamically and petrographically identical with those that transform, say, a flat lens of calcareous shell sand into a sharply compressed syncline of limestone at the temperatures and pressures that prevail at a depth of a few kilometers below the earth's surface. Ice, after all, is a rock, i.e., an elastico-viscous, inorganic, crystalline aggregate. As such, it exhibits elastic properties under the instantaneous impact of an earth tremor or of the geologist's hammer, but flows quasi-plastically under prolonged stress. In ice, as in rock, the solid flow is the result of three processes that go on simultaneously under continued stress: crushing and turning of individual crystal grains; deformation of crystal lattices by gliding along crystallographically defined "gliding planes"; and, above all, by transfer of ions from stressed surfaces to faces subject to less stress. As a result, the whole crystal fabric of the glacier ice undergoes changes that are identical with those found in rocks that have suffered mechanical deformation. This is shown by the optical study of thin-sections and the statistical method of "petrofabrics", which was applied to glacier ice in Europe by Bader, Perutz and Seligman, Winterhalter and others, and in the United States first by Max Demorest, my lamented friend and former student. Such studies are now recognized as an essential part of modern glaciological research. But it is not generally understood that in ice, as in rock, such microscopic study of thin-sections must be supplemented by a careful analysis of all megascopic structural features. What are such features?

First, in ice, as in rock, permanent deformation generally results in folding. Where visible folds are produced on the surface of the ice, as in the case of two floating ice sheets pressing against each other in the Bay of Whales, they duplicate accurately the folds that are produced on the earth's surface by mountain-making forces. They can and, where intensive studies are undertaken, they should be mapped like their geological counterparts. This means plotting the trend of the fold axes, their plunge, typical dips on both flanks, the attitude of the axial plane and, where the stratification is sufficiently distinct, variations in thickness on crests and limbs.

When the flat sheets of the névé of an ice field squeeze down into the narrow confines of a steep-sided mountain valley, the effect is the same as if the ice sheet were being compressed laterally into a syncline. Where tributary glaciers flow together in a major valley, juxtaposed or superimposed, their ice bodies assume the characteristic shape of anticlines and synclines. The rapid wasting of valley glaciers exposes this

structure at the surface in the same way in which erosion brings out the corresponding structure pattern in folded rocks. Along the "anticlinal" axes, the lowest layer, laden with débris, comes to the surface, while the highest preserved level of ice occupies the "syncline". In addition, the longitudinal slope that is produced by the wasting of the ice surface, cuts the internal structure obliquely and exposes the primary stratification of the ice in the characteristic pattern known as "ogives" and "plow furrows" (the "Ackerfurchen" of Crammer).

A careful three-dimensional analysis of the fold structure, based on detailed field mapping, furnishes quantitative information on the changes of shape that the ice body has suffered. Since the details of this structure are sufficiently recognizable only in a few of the glaciers of a given region, their development should be one of the criteria that determine the choice of a glacier for prolonged, intensive study.

Differential rates of flow in adjoining ice streams of a compound glacier produce "drag folds" along the borders of medial moraines. Adequately studied and mapped and dimensionally fitted into the dynamic picture of the flow of the glacier, they may well, when more is known about the viscosity of glacier ice, yield as much quantitative information as many years of mere observation.

Planes of fracture and shear constitute the other group of important structures exhibited by ice that has undergone deformation. Crevasses and blue-bands produced by shear have their geological counterparts in the joints, faults, and thrusts of folded rocks. They are by no means accidental and purely surficial features. Plastic flow, shearing, and fracturing go hand in hand, at depth as well as near the surface, at the rates of movement that prevail in the descent of glaciers and the folding of mountain ranges. The planes of fracture and shear that come into being bear definite geometric relations to the axes of greatest, intermediate, and least elongation which are implied in the change of shape of the deforming body of ice or rock. One reflects the other;therein lies their potential value for glacier studies.

In this connection it is important to distinguish between what I have called "intrinsic" fractures and "locally imposed" fractures. The former spring into being in sets of parallel planes throughout the body. The latter are unique, due to the accidents of shape and contact. The cracks that appear all over the surface of a rock cylinder that is being compressed in a testing machine, are examples of "intrinsic" fractures. The crack that forms when a bolt that holds together two steel plates is sheared off, is a "locally imposed" fracture.

A brief statement of two basic principles shall serve to call attention to the possibilities of a systematic analysis of the fracture patterns of glaciers. A fuller exposition of this field of structural geology is now being prepared for the press.

At a given point in a body undergoing permanent deformation, fractures can form in three, and only three, positions with reference to the principal axes of strain existing at the time at that point. Two possible conditions of strain must be distinguished: (A) The first may be conveniently represented by a cube that is deformed in such a way that the length of its sides remains unchanged. As the deformation progresses, the area of the cross-section, shown in Figure 1, decreases. This is possible only if the material contained in the cube is free to elongate at right angles to the plane of the cross-section. In this case, then, two of the principal axes of strain lengthen, while the third shortens. We may express this by saying that the axis of greatest elongation (e) and the intermediate axis (i) are positive, while the third, (k), is negative. (Figure 1).

Under this condition, fractures form either at right angles to "e", or in a pair of conjugate planes that are inclined roughly 60 degrees to the "e" axis and intersect in a line that is parallel to "i". These are the "tension fractures" and "oblique shear fractures" of geological literature. The latter never intersect at right angles and, of course, never at angles less than 90 degrees in the direction of greatest elongation. On the whole, the more readily a material ruptures, the more it tends to develop tension fractures rather than oblique shear fractures.

In a glacier, the common types of crevasses are all tension fractures: the marginal, terminal, and transverse crevasses. Many air photos of valley glaciers as well as of the Antarctic shelf ice suggest the presence also of oblique shear fractures. But their interpretation is still in doubt.

Whether intersecting fractures are really conjugate shear fractures or merely tension fractures produced at different times under different conditions of strain, can be decided only by comparing their geometrical position with that of the direction of greatest elongation established by independent evidence. In deformed rocks, this is possible in three ways: (1) In many rocks, the direction of greatest elongation is clearly defined by the shape of stretched oolites, pebbles, fossils, and even lava pillows. (2) In sedimentary rocks, it is common for one of two successive layers to have fractured under tension, showing but one set of joints, while the next layer developed two sets of fractures that lie symmetrical with reference to the single set of the preceding bed. Such symmetry, often repeated in many layers, fixes the direction of greatest elongation conclusively. (3) Finally, the general shape of the rock folds, seen in three dimensions, permits less specific, but still very useful conclusions concerning the over-all position in space of the major strain axes. The last case alone applies to glaciers.

(B) In the second and only other possible condition of strain, there is shortening, or at least no lengthening, in the direction of two axes, "k" and "i" and elongation only along "e". This case, shown in Figure 2, implies a stretching of at least two of the sides and differential movement parallel to the direction of greatest elongation. It produces shear planes that lie parallel to "e" and normal to "k". They might be called "parallel

shear planes". In rocks, they are the planes of flow and fracture cleavage. In ice, as in many metals, shear is apt to produce zones of uniformly oriented crystal lattices, developed through gliding, rather than shear fractures. Such is the origin of the closely spaced longitudinal blue-bands that are seen in many glaciers that have been subject to abnormal squeezing. This was recognized more than a century ago by Guyot who compared them to the rough cleavage exhibited by calcareous schists. Such is also the origin of the more or less spoon-shaped blueband - shear planes that dip gently upstream near the end of a glacier tongue and behind every obstacle on the bed of the glacier that induces "extrusion flow".

Only one more principle needs to be mentioned here. As deformation progresses, the fracture and shear planes themselves become deformed. Some may gape open; others will shut tightly; straight planes may be bent; opposite sides of fractures will move past each other and in doing so will change the position in space of the fractures and the angle between them. Fortunately, all changes in the position of and the angle between fracture planes off-set most other structure lines, such as stratification and bluebands. That changes in the relative position of fracture planes are always recorded by off-sets in structure lines constitutes an important principle. All such changes can be studied profitably by watching the corresponding transformations in the fracture patterns that form in masses of wet clay, subject to deformation by devices such as have proven so effective in the hands of Hans Cloos in his studies of "artificial mountains". Experience gained with the aid of such laboratory experiments develops the understanding needed for the analysis of the complex fracture pattern seen in mountains and in glaciers, in terms of the magnitude and direction of the strains that produced it.

Vertical air photographs are indispensable for the systematic study of the structural features of a glacier. On them all field observation should be recorded with the aid of the symbols and conventions used by the field geologists. Photographs of the same significant stretches of a glacier chosen for intensive study, taken at adequate intervals and supplemented by repeated field studies, will lead to an insight into the successive stages of the deformation that fashioned the glacier tongue from the parent névé that cannot be obtained in any other way.

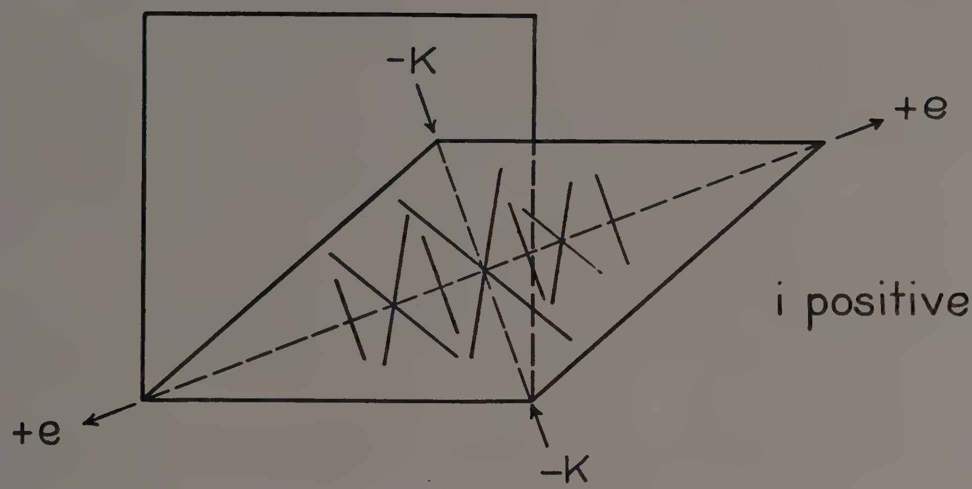

FIG. 1—Diagrammatic section of a cube undergoing homogeneous deformation such that the length of sides remains constant and two of the principal axes of strain lengthen. It shows the position of "tension" and "oblique shear" fractures with reference to the direction of greatest elongation (e). (The small letters (i) and (k) stand for the intermediate and least axes of strain, the former being normal to the plane of the paper.)

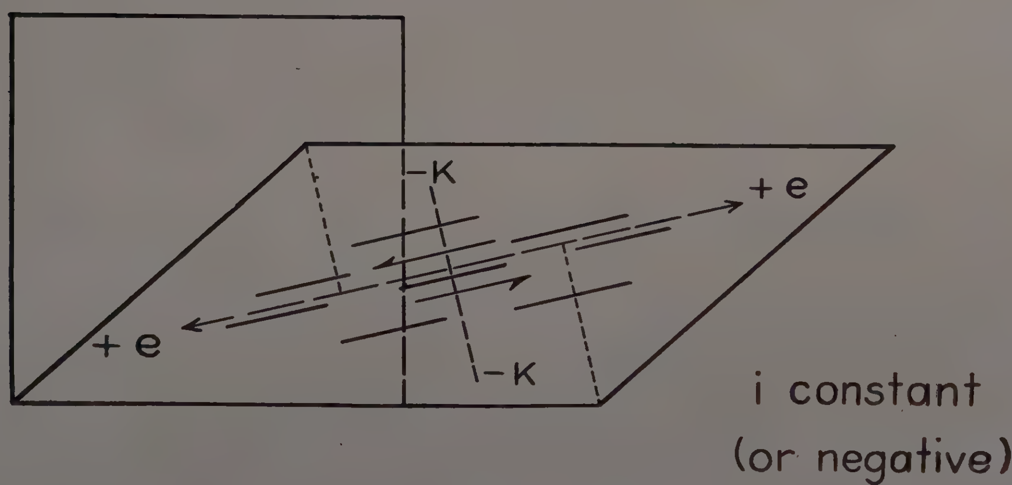

FIG. 2—Diagrammatic section of a cube undergoing deformation such that the length of two parallel sides increases and only one of the three principal axes of strain lengthens. It shows the position of "parallel shear" fractures with reference to the direction of greatest elongation (e).

Dr. Dyson was then asked to outline the nature of his studies of glaciers in the Glacier National Park area. He emphasized that these relatively small ice bodies afford an excellent opportunity for observations of structure, ice motion, moraine patterns, and changes accompanying variations in volume. He stated that during the next few years it will be possible to observe the disappearance of some of these glaciers and then, should the present climatic trend reverse, we probably would be able to witness their rebirth and subsequent growth in the same cirques. His studies also revealed that openings of significant size occur within the basal portions of actively moving glaciers where the ice may be three hundred or more feet in thickness. Such openings indicate that plastic behavior of the basal ice, at least within these small cirque glaciers, is local.

The ensuing discussion brought out the advantages of selecting such an area for certain types of detailed studies and the need for more systematic observations among those rapidly shrinking glaciers.

Four current field projects either underway or planned for the near future were described:

1) The Arctic Institute's 'Project "Snow Cornice"' was discussed by Mr. Wood, organizer of the undertaking and leader of the 1948 expedition. The glaciological program is under the direction of Dr. Robert P. Sharp, Division of Geological Sciences, California Institute of Technology. Since this project had been described at the meeting the previous evening, only its principal features were reviewed. Published accounts have already appeared as follows:

Wood, Walter A., 'Project "Snow Cornice": The Establishment of the Seward Glacial Research Station'. Arctic, Journal of the Arctic Institute of North America, vol. 1, no. 2, Autumn, 1948, pp. 107-112.

Sharp, Robert P., 'Project "Snow Cornice"', Engineering and Science Monthly, Published by California Institute of Technology Alumni Association, November, 1948.

2) Mr. Baird described his Baffin Island Ice Project for 1950, which is sponsored by the Arctic Institute. This expedition to the interior of Baffin Island will include studies of what appears to be a remnant ice mass, measuring about 60 miles in length and 25 miles in width, occupying relatively low ground in about Latitude 70 degrees North. Most interesting aerial photographs of this ice field were passed around. Although the detailed plans for the glaciological program have not yet been worked out, he indicated that observations would be made of the relative areas of ablation and accumulation, the position of the névé line, flow mechanism along the periphery, the growth of ice crystals in this typically arctic climate, the temperature conditions existing in the ground under the ice margin and in areas recently uncovered, and the pressure of air bubbles

in the ice which may indicate whether they have been subjected to the great pressures which would be present at depth in an ice sheet. Such meetings as this, he hoped, would aid in planning a well-integrated glaciological program.

3) Mr. Field described the American Geographical Society's "Glacier Study Project" which includes observations in North America, particularly in the Juneau Ice Field area of Southeastern Alaska, and in the Andes of South America, - taking advantage alternately of the Northern and Southern Hemisphere summers. The first field trip was undertaken in September, 1948, when a party of six under the leadership of Maynard M. Miller and William R. Latady undertook a reconnaissance of the Juneau Ice Field and three of its outflowing glaciers. Routes of access were determined, camp sites located, and a start made in the glaciologic, meteorologic, geologic, and botanic studies. The program will be continued with both high and low level studies of the area during the summer of 1949.

The work in South America will begin with a reconnaissance trip to Argentine Patagonia from February to May, 1949. The field work will be a joint undertaking by the American Geographical Society and the Museo Argentino de Ciencias Naturales "Bernardino Rivadavia" in Buenos Aires. The Society will be represented by Dr. Robert L. Nichols and Maynard M. Miller and the Museo by three Argentine geologists of its staff.

4) Mr. Field spoke briefly of the program of the Research Committee on Glaciers, Section of Hydrology, American Geophysical Union. The main purposes of the Committee were mentioned and the scope of its functions described.

This concluded the first half of the conference which consisted primarily of prepared statements.

In opening the second part of the conference, the Chairman asked Dr. Shelesnyak to say a few words.

Dr. Shelesnyak: "I should like to comment on the broad problem of the acceptance of research in the field as a valid contribution to the American academic scene. On the whole, social, physical and biological studies in universities which are in great need of field counterparts to laboratory investigations for their fullest understanding, are not looked upon with favor in so far as the field activities are concerned. Although great support and high respect is given to the conduct of work in the laboratory, supplementary or even complementary work in the field is relegated to secondary importance. Permission to work in the summer recess of course is given, provided no summer school teaching is required, and occasionally a Sabbatical year allowing the investigator to go into the field. But as a definite pattern, the stimulation and the promotion of research in its natural environment in the field proper and its acceptance by the academic fraternity as a truly valid operation both for training and for research is, unfortunately, not as well established as it should be. Fortunately, there are some disciplines where the field is as acceptable an area for study as is the laboratory - geology and glaciology among them. It should be urged that such as are fortunate enough to be associated in those fields stimulate this viewpoint for other sister sciences. From the point of view of Government support of research and Government interest in research, this is extremely critical, since in a large measure many of the problems, whether they be for national security or for national development, are essentially problems in the field and demand field work-trained men, a type of individual which is difficult to find unless the university is going to train him."

He then discussed the matter brought up earlier by Dr. Bader as to the amount of time which is consumed in "housekeeping in the field". He said that without careful comparative analysis of activity involved, it would be difficult to match the "housekeeping factor" in the field with the "housekeeping factor" in an environment such as the university laboratory, where one is forced to live, housekeep, cook, and move the laboratory about. To be critically severe about this housekeeping aspect in field work might well discourage the development of field work in general. Actually, if one would stimulate a more receptive attitude towards field work in this country, the "housekeeping factor" would become increasingly smaller.

He also spoke of the concept of scientific research by and in the military services. Since basic research is a relatively new role in the military, there are a great many problems of an administrative nature which must be considered - "the quality of military research, especially of the military laboratories, is more in the hands of civilians insofar as directing, nurturing, and influencing than it is in the hands of military top structure. In other words, if the scientists in civilian areas (universities particularly) would concern themselves more with the research problems of the military, not military research, that is, not the military problem, but the scientific problems which the military are attempting to

tackle, then I believe we would have a much happier growth in the military organization of scientific personnel".

He then described the laboratory established by the Office of Naval Research at Point Barrow, Alaska, for the conduct of basic research. The laboratory has about 80,000 square feet "where facilities are available for all types of research, physical and biological. Encouraging aspects are that in the first year, there were eight scientists; by the end of the first year, going into the second, there were between sixteen and seventeen for the whole season, with a summer peak of 35. By next August (1949) it will be three years from the first piece of paper suggesting the establishment of the laboratory." He added that it was an excellent place to work in such fields as the study of sea ice and fresh water ice, and that it brought a well-equipped laboratory into the field. Plans are underway for further expansion of the laboratory during the coming year. [This laboratory has been described in considerable detail by Dr. Shelesnyak in "The History of the Arctic Research Laboratory, Point Barrow, Alaska", Arctic, Journal of the Arctic Institute of North America, vol. 1, no. 2, Autumn 1948, pp. 97-106.]

Dr. Ray: "I think we are very fortunate to have the interest of the Armed Forces in this type of work because we have been provided with many photographs and other data, as well as opportunities to get into areas which would otherwise be inaccessible." He added that this help has also given valuable stimulus in a field which has been largely neglected and has provided publicity and backing which has long been needed and is extremely welcome.

Mr. Field: "This last point is an extremely important one which should be emphasized. Both the Arctic Institute's 'Project "Snow Cornice"' and the American Geographical Society's 'Juneau Ice Field Research Project' have received valuable support from the National Military Establishment. Other projects have also been greatly benefited."

Mr. Miller: "Speaking for the Juneau Ice Field Research Project, we are most grateful, both to the Departments of the Navy and the Air Force. The Navy's Medium Patrol Squadron Four, which last summer was engaged in an aerial photographic survey of Southeastern Alaska, cooperated closely with our glacier study project. Besides taking a large number of aerial photographs specifically for our project, the Commanding Officer of this unit, Commander Thomas F. Pollock, provided invaluable air support by dropping nearly a ton of supplies and equipment at designated high level points on the ice field. Through the Research and Development Board, the Air Force also provided a plane to transport equipment and personnel to Alaska."

There ensued considerable further discussion of the problem of time spent in the field on living requirements (housekeeping) and logistics. Mr. Miller spoke of the difficulty of doing good work on an ice field when living conditions are cramped and there are no adequate lighting or cooking facilities. He pointed out that a party's morale and efficiency suffers when tents are uncomfortable, sleeping bags are wet, and hands and

feet are cold. If relatively good living conditions are provided, he thought that the 60 percent of time spent on housekeeping [reference to Dr. Bader's paper] could be reduced to 30 percent, and that just as good careful work could be turned out in the field as in a college laboratory.

Dr. Nichols: "The ratio of time spent in productive field work to the total time spent in the field is of interest to geologists, geographers, botanists, archaeologists, and other field workers. There are many factors which control this ratio, among which the following are important: (1) the climate (rain, snow, cloudiness, temperature, hours of sunlight); (2) the nature of the terrain (relief, vegetation); (3) accessibility and distance from the office; (4) time spent in living (getting food, water, and supplies; cooking, eating, making camp, sleeping); (5) time spent in labelling, preparing, packing and shipping specimens; (6) the personal equation (the physical and mental energy of the leader and assistants); (7) the size of the field party.

"Dr. Hugh M. Raup, a field botanist, once told me that for every day he spent collecting plants, he had to put in two days drying, pressing, and getting them ready to ship back to the laboratory. Dr. J. Peoples spent several field seasons doing geologic work in an area in Montana where there was 3000 feet of relief. The work was so strenuous that he and his associates had to rest one day for every two days of field work. On a 90-day geological sledge trip which I took in the Antarctic, only 20 days were spent in productive field work. Considering the time spent in getting to a remote area and the other factors involved, a geologist should consider himself lucky if his ratio is as good as 1 to 10. This ratio is of particular concern to the leader of every field party and one of his big problems is to determine which course of action will result in the lowest ratio."

Mr. Wood: "One thing is important in this discussion of how much time is wasted in the field, particularly to those of you here who are connected with institutions of higher learning. If we are going to deal with glacier studies, there is no greater contributory factor to loss of efficiency than to send into the field men who are unqualified to travel that particular type of terrain. We learned that very strongly last summer. In our party we had a team who were laboratory technicians - they had developed their research methods within the laboratory, but were totally unfamiliar with field techniques. In fact, they were working under a great disadvantage. I cannot emphasize sufficiently the need for those who are training now to go into this field, to become familiar with the necessary means of transportation and accustomed by experience to the environment in which they are going to work. Through such preparation, they will find that effiency of field work will be greatly increased."

Mr. Dobrin: "As a member of the staff of the laboratory at Point Barrow to which Dr. Shelesnyak referred, I would like to testify as to the facilities which are offered there for almost any kind of scientific undertaking. The work I was doing was geophysical, which usually takes a good

bit of transportation, shop facilities, and electrical equipment. The availability of material there was greater than it would ordinarily be in most places in the United States. Dr. Shelesnyak has pointed out that if you want to get some workmen to help you in Washington, you spend two or three weeks cutting red tape in order to get them, whereas at Point Barrow you may have them the day you ask for them. That is largely because of the fact that the Petroleum Reserve has these facilities for oil exploration, but inasmuch as they are also available for scientific work, that ought to be considered by anyone who is looking for a place to work in the Arctic. The little time spent on the general mechanics of living is a minimum compared with any scientific work I can think of anywhere. One has to spend no time whatsoever of his own for living except for eating and sleeping. The sleeping facilities are about 500 feet from where you work and the dining hall about 200 feet. The rest of the time, as many hours of the day as you wish, are spent working. I think the ratio you can establish of living time to working time is almost infinitesimal here as compared to any other place."

The Chairman then asked for a discussion of current glaciological activities and the various field techniques involved.

Mr. Wood outlined the proposed glaciological research program of 'Project "Snow Cornice"' for the field season of 1949 by reading the following excerpt from Dr. Sharp's report to the Office of Naval Research, dated December 2, 1948:

Program of Investigation

Radar soundings. - The attempt to use radar for determination of ice thickness in glaciers in 1948 gave encouraging results, and further studies with modified and improved equipment are given high priority. A reasonable transverse profile of a valley glacier was obtained showing a maximum ice thickness of a little over 700 feet. The ultimate depth limitations and reliability of this method have not yet been determined, but the mobility and ease of operation of the apparatus compared to other methods of sounding through ice will justify further work.

Seismic soundings. - Work with seismic equipment on both the Seward Ice Field and Malaspina Glacier is desirable for several reasons. First, a reliable check on the radar results is absolutely necessary before that method can be proved out. Seismic soundings appear the best and most reliable means for checking the radar observations. Second, a number of profiles across both the Seward Ice Field and the Malaspina Glacier is essential to an understanding of the mechanics and mode of flowage in these ice bodies. The thickness of ice in parts of the Seward Ice Field is thought to be at least 2500 feet, and this appears to be too great to be measured in any other way than by seismic sounding. Third, the adaption of seismic surveying equipment to an operation of this type is

well worthy of study in its own right. In spite of previous uses of seismic equipment on ice by Wegener, Poulter, Goldthwait and others, much remains to be done along this line.

Physical Glaciology. - Studies of the physical nature of firn (névé) and ice started in 1948 will be continued in 1949 with further investigations of temperature, density, meltwater volume and its mode of circulation, free-water content, and structures within the firn and ice. Further studies of rates of glacier movement will also be made.

Cryology. - A cold laboratory will be set up at Yakutat airfield for the use of Dr. Henri Bader in his study of crystallography and fabrics of specimens from the Malaspina Glacier and Seward Ice Field. Dr. Bader will also carry on field studies along these lines. It is hoped that his work will contribute to a further understanding of the structures and stresses in the ice, and this should lead to a better comprehension of how ice yields to these stresses by flowage and fracture.

Nourishment and wastage. - More information on glacier nourishment in the Seward Ice Field was obtained in 1948 than had been anticipated. Further work directed toward assessing the total "glacier budget" in this area is justified. The results and observations of 1948 indicate that even such experienced glaciologists as H. W. Ahlmann have misjudged the climatological environment of this area and therefore have had a misconception of the mechanism of ice wastage and nourishment here. These studies will help fill out the world-wide pattern of similar investigations that is slowly evolving. The Seward Ice Field occupies a prominent position near the north end of a long line of glacier-bearing mountains stretching from Alaska to the southern tip of South America. Glaciological studies along this mountainous chain should eventually give information as to relative behavior of glaciers in the northern and southern hemispheres. This is a matter of first importance to theories concerned with the ultimate cause of glaciations and ice ages.

Micrometeorology. - It is hoped that proper personnel will be available to institute a program of micrometeorological studies on the Seward Ice Field. Principal interest will lie in those factors controlling the wastage (largely by melting) of ice and firn during the summer. Studies should be made of radiation, temperature-conduction, temperature and humidity gradients, and air movements in the layer a few feet to a few tens of feet above the surface of the ice field. This work can be related to the program of study already carried out by Sverdrup in the northern Atlantic regions. Naturally, meteorological observations of other types would also be encouraged insofar as feasible, for this would be an excellent opportunity for gathering meteorological data in a remote mountain area.

Dr. Ray: "It seems important that one of the things that should result from such studies is the correlation in specific areas between years of heavy snowfall and slight advances of the glaciers. As yet our meteorological data and records of glacier fluctuations are not adequate, but in time it should be possible to establish the causal connection. This is one objective that can result from a long term study."

Speaking as one whose field of interest is primarily in glacial geology rather than glaciology, he stressed the importance of determining in all these studies the kind of glaciers being observed, whether temperate or Polar and the nature of the ice, for the properties are going to vary and one set of observations applicable to one type of glacier may not be applicable to another. He also suggested that it might prove worthwhile to make a chemical analysis of the water, in the upper layers as well as at lower levels.

Mr. Miller then asked if it would be possible for the Geological Survey to make observations of stream run-off and an analysis of the waters from glaciers such as the Lemon or Mendenhall near Juneau, Alaska, to tie in with the ablation and meltwater studies being made on the Juneau Ice Field above.

Dr. Ray: "I cannot say now whether this could be done. I know from personal experience that fluctuations in some of those streams - seasonal and daily fluctuations - are very large. Several years ago, I formulated a long-range project which was to have involved some ten years of study in either the Juneau area or the Valdez area with the establishment of a laboratory for year-round study. But because of the general lack of personnel and the press of other business, such a project has never been implemented and I have my doubts whether it will be, at least in the very near future. However, it seems to me that such studies are extremely important. It also seems there should be more work along the lines Dr. Bucher has mentioned, which was one of the principal objectives I had scheduled in the proposed program which died in the files."

Dr. Washburn: "We have been presented with two apparently similar research programs - "Snow Cornice" and the Juneau Ice Field Research Project. I would like personally to hear some discussion as to the similarities in the programs and as to the differences, with a view to obtaining an over-all picture. Both programs are located in Alaska, yet occupy quite distinct geographic positions in terms of altitude and accessibility. I should like to know if the programs can be closely integrated and what studies can be carried out only by the one or the other."

Mr. Miller answered by mentioning several important differences: (1) the need of carrying out detailed studies from the lowest to the highest point on the glacier, concentrating especially in the névé or firn line zone. This can be done on the Juneau Ice Field and its outflowing glaciers, whereas in the Seward-Malaspina Glacier area, one can make low level and high level studies, but it is very difficult under present conditions to

reach the intermediate area in which is situated the névé line; (2) in the vicinity of the Juneau Ice Field low level glaciological and meteorological observations have been carried on at several stations for 50 to 75 years. This data adds immeasurably to the significance of the present detailed studies at higher levels; (3) the accessibility of the Juneau area and the comparative ease with which all areas of the glaciers can be reached; (4) the Juneau Ice Field may be more continental than the Malaspina-Seward area and there is evidence of chinook winds on its eastern slope; (5) in the Juneau area there is the unusual phenomenon of an advancing glacier, the Taku, which is one of the most interesting glaciers in North America. Its behavior is known since the early 1890's and is in need of adequate explanation; (6) the Seward-Malaspina area has more arctic and alpine characteristics and is considerably higher. In that way it offers an almost different zone for study; (7) certain structural features appear to be different in the two areas. For instance vertical ice columns in the névé were only seen on the Seward Glacier between 5000 and 7000 feet, while on the Juneau Ice Field they occurred at a much lower level. In summing up, Mr. Miller said: "There are these differences which make these two regions unique and distinct and at the same time, I think, correlative in glaciological, meteorological, geological, and botanical aspects. There is a great need for all the coastal ranges of ice-covered mountains in Alaska and the adjacent parts of Canada to be studied and compared, especially at higher elevations. Since the two areas under discussion are 250 miles apart and are quite distinct, there is good reason for an integrated study of both."

Mr. Wood: "I agree 100 percent, but in view of the fact that Dr. Sharp is the scientist in charge of these investigations for the Arctic Institute, I think he is the person who should speak on the particular point that Dr. Washburn has raised."

Mr. Baird: "I would like to say one thing about this; perhaps interpret some of Professor Ahlmann's views on the subject. That might briefly be - "the more the merrier". We do have in the projects suggested today, completely different types of glaciers as Ahlmann defines them. The Juneau Ice Field, Ahlmann would define as high-land ice. The "Snow Cornice" area possesses two types - a valley glacier on the most enormous scale with a large accumulation area, and the piedmont glacier of the Malaspina. And the project of mine in Baffin Island is another type again, according to Ahlmann, - the true ice cap. So they fall into totally different classifications of glaciers and I think this continent should be able to support research on all of those types of glaciers."

Mr. Miller asked for information on the possibility of using gravimetric equipment for determining the relative highs and lows of the rock floor beneath glaciers and ice fields and what other geophysical equipment might be used for measurements of the thickness of ice.

Mr. Worzel answered that he did not believe gravity measurements would be satisfactory, because as a preliminary you have to know the

surrounding gravity field which is a long project in itself to determine. He then suggested that the method developed by Dr. Poulter in Antarctica, which has proved very satisfactory in prospecting for oil, should be attempted on glaciers. He then added: "My general feeling is that seismology is going about it from the wrong end. We need to know a good deal about velocities before we can use seismic methods very effectively. You need to measure refraction seismic long before you can do anything successfully with reflection seismic, and that involves shooting large charges and it involves a great deal of equipment to be packed in and so forth. But I do not think you will get results until you do do it."

In answer to a question from Dr. Nichols as to how travel times could be expected to vary with the density of the snow over ice and the number of crevasses per unit area, Mr. Worzel said he did not think that crevasses were of any great significance any more than the fracture of the rocks in the Earth. Ordinarily, he thought that those factors averaged out in the shooting. He did believe that density variations would be significant but to what extent he did not know because so few experiments had been made on ice and its variations. The only seismic experiments he knew of which had been really successful were refraction measurements of ice on small lakes where there is not much density variation.

<u>Dr. Nichols</u> then asked how Mr. Worzel would comment on Wegener's work on the Greenland Ice Cap in view of the fact that travel times are not known.

<u>Mr. Worzel</u>: "If you would make some assumptions, you can say that the ice field thickens or something like that. You may be off by a factor or two perhaps in the total thickness, but you can say how it varies."

<u>Mr. Dobrin</u>: "I wonder whether the people who have done this type of work have had access to the literature on the subject of seismic waves in ice. I am not well acquainted with it, but it is summarized in the book, "The Internal Constitution of the Earth", which has about two pages of comment on various work that has been done on the velocity of sound in glaciers in Switzerland and Austria. One thing I noticed just from looking this over very hurriedly, is that the values of velocities do seem to be pretty close together as determined in various glaciers in Greenland and the Alps, so that if possibly there were no way of measuring it in the field, one could use these as a first approximation. In the second place, there are standard techniques for determining velocity as it changes with depth in an entirely unknown region by the usual refraction methods used by oil companies for prospecting. In general, the biggest problem we seem to have in ice is that the ice itself is pretty shaky and unstable, and that one has a lot of background on the records. Sometimes if signals are weak, it is hard to determine the arrival times precisely."

He then inquired whether those considerations had been taken into account in 'Project "Snow Cornice"' on the Seward Ice Field the previous summer.

Mr. Wood: "That problem never arose, unfortunately, on the Seward Ice Field. The seismic team expected they would be set down on something which was a cohesive body of ice and that they would apply the Goldthwait-Washburn techniques to this penetration. Consequently, they were slightly jarred at the need of obtaining velocities in a medium graduating from firn into ice which had never occurred to them."

Dr. Bucher: "One set of observations by the seismic refraction method was made on the Ross Shelf ice by Colonel Charles Gill Morgan as chief geologist and geophysicist of Byrd's second Antarctic Expedition (1933-1935). He found an average density of 0.4 - 0.5 in the uppermost (névé) layer, to a distance well below water level. Further down the density increases, but nowhere does it exceed a value of 0.85. Unfortunately, the tests were preliminary and the records have not been published. Dr. Maurice Ewing and I are in agreement that in order to be considered in the future, any seismic results that are published should include copies of the seismic record, plus details concerning their evaluation."

Mr. Dobrin: "In answer to Mr. Miller's question about any other method that occurred to anyone here in determining ice thickness, there is one geophysical method which is not entirely promising, but which I think ought to be tried. That is the method of electrical resistivity. In a way, it is a rather clumsy method, particularly if the thickness of the ice is at all considerable; on the other hand, it is a very inexpensive method and sometimes could be used in regions where the seismic method might be just too involved and expensive, taking too many men and too much equipment. The resistivity method has been used as a standard technique for determining the thickness of overburden when there is alluvium or gravel overlying the hard rock; it takes advantage of the difference in conductivity between two strata. The upper stratum would be one, say, of low conductivity, and the lower stratum of high conductivity or vice-versa. By measuring the electrical resistivity systematically, you can determine the place where there is a discontinuity between two conductivities. The exact technique need not be gone into here. If it turned out that there are any differences in electric conductivity between your fresh water glacier ice and the rock below, and this could only be determined by experiment, I think you might possibly have a good method for determining thickness where seismic means would not work or would not be available - provided the thickness of ice is not too great. The method has been used to study permafrost and has been reasonably successful. The ice on the Arctic Ocean is only about four or five feet thick at the most; it may be just a few inches thick when the ice is just breaking up and when ice is just forming. Most of the transportation in the Arctic is over the ice along the shore because it furnishes a smooth surface, and the problem arises, whether it is safe for a caterpillar tractor or a weasel to go out over the ice. Ordinarily, if a hole must be drilled in the ice at each point to determine whether it is thick enough, it would be very tedious and time-consuming. We were trying to develop a method by which we could just drag along something on the ice and determine thickness at any time. Theoretically, if the ocean ice had no salt water in

it, and the water below were entirely salt, then you should get a good discontinuity which would show up. It happened, however, that the ice had too much salt in it, so that it was not possible. Anyway, that method might be tried in glacier work."

Mr. Baird asked to what thickness of ice Mr. Dobrin thought this method might be applied.

Mr. Dobrin: "I think it has been used on permafrost down to 300 or 400 feet."

Mr. Miller then brought up for discussion the problem of drilling into the ice for temperature measurements and subsurface structural and petrographic studies. On the Seward Ice Field in 1948, a hot point had been used and a depth of 204 feet attained. He asked for advice on techniques and equipment and posed the question whether diamond drilling cores might be used to depths of 5000 feet on an ice cap such as in Greenland.

Dr. Nichols stated that Paul-Emile Victor's Expédition Polaires Françaises was going to do some drilling on the Greenland Ice Cap and was bringing in a lot of pipe during the summer of 1948 in preparation for the field season of 1949.

Dr. Bader remarked that at a certain depth "around a thousand feet perhaps", it will probably not be possible to drill further "because the ice under that hydrostatic pressure is so plastic that the hole closes as fast as it is cleared. There is no doubt that you are going to reach a depth - I don't know where it is - where you will not be able to do any more mechanical drilling."

Considerable discussion followed in which Mr. Wood, Dr. Bucher, Dr. Nichols, Dr. Dyson, and Mr. Miller took part. The consensus of ideas was that the matter should be studied and that the experience gained in drilling oil wells to great depths should be taken into account.

Mr. Miller emphasized the need in this country for better access to foreign sources of information on the various aspects of current glaciological activities. He expressed the hope that out of this meeting might evolve a center where such information could be coordinated for the use of all students of the subject instead of the present widely scattered distribution of source material.

Dr. Washburn: "Now, that is the desirable sort of thing the Arctic Institute is trying to do with regard to Arctic research in general; to make sure, for instance, that papers published in Europe are sent to the Institute."

Dr. Ray: "There is also a committee of our National Research Council devoting its energies primarily to the Pleistocene. Its members are going to make an attempt to see if it will not be possible to produce some sort

of annual bibliography, and they have requested that the various papers coming out be sent to them. Dr. Bader is a member of this committee and so, probably, many things will be sent to him, and these references will be made available to all those concerned with this study.

Dr. Bucher cited as an illustration the fact that a few days before, he had come across a reference to "Gletscherkunde" by Drs. Erich v. Drygalski and Fritz Machatschek, published in Vienna in 1942. No copy is available to him and yet it is probably one of the most important works on glaciology of recent years.

Mr. Field stated that the Committee on Glaciers planned to send to its members and those on its mailing list notices of new publications, and expressed the hope that separates of all papers published might be made available for the Committee's permanent files.

Mrs. Orcutt expressed the hope that different scientific libraries might cooperate.

Dr. Ray agreed and cited such libraries as that of the U. S. Geological Survey, the Library of Congress, and the big university libraries.

Dr. Bader emphasized the need for systematically reviewing the literature and assembling bibliographies, and mentioned his own reference file which already includes some 500 items.

Dr. Washburn spoke of the Arctic Institute's bibliographic project dealing with matters bearing on the north, including snow and ice research, and said any help from glaciologists would be very much appreciated. He said Miss Tremaine had a full staff working on this Arctic bibliography at the Library of Congress.

Dr. Bader emphasized once again the need for a physicist to review all existing literature dealing with the physics of ice, who would then write up a summary of his findings of the present state of our knowledge and theories. He would have to have access to the work which has been done in Europe and which has been written up in foreign languages. He estimated it to be at least a one or two year task, but very worthwhile and essential to those who are studying and experimenting in the whole problem of ice mechanics.

Dr. Nichols suggested it might be better to have a man familiar with ice study its physical aspects, rather than to get a physicist to take up ice problems.

In conclusion, those present agreed that this gathering had been useful in providing an exchange of ideas on glaciology, and expressed the hope that it would be possible to hold more such conferences in future years.

The meeting was then adjourned at 6:00 o'clock. A summary of the main points of the discussion during the second half of the meeting, aside from specific techniques and methods, would include the following:

1. The need to apply advanced techniques in the detailed study of certain designated glaciers, such as already undertaken in the Alps, Scandinavia, and the North Atlantic area, and which were begun in 1948 on the Seward Glacier and on the Juneau Ice Field in Yukon Territory and Alaska.

2. The need for encouraging studies in the physics of glaciers and a correlation of all previous studies and experiments.

3. The desirability of building up an information center in North America at which papers and reports of glaciological work being done in various parts of the world could be collected and made available to those working in that field.